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ABSTRACT

This issue of the IRCD Bulletin is devoted to commentaries on an article by Arthur R. Jensen on the nature-nurture controversy in a recent number of "Harvard Educational Review." Contents of the Bulletin are comprised of the following: "Education, Ethnicity, Genetics and Intelligence; Jensenism: Another Excuse for Failure to Educate" (Edmund W. Gordon); "What Jensen Article?" (Carol Lopate and Edmund W. Gordon); "A Comment on the Jensen Essay" (Benjamin S. Bloom); a statement by the Council of the Society for the Psychological Study of Social Issues entitled "Psychologists Comment on Current IQ Controversy, Heredity versus Environment"; "A Flaw in Jensen's Use of Heritability Data" (Allan C. Goldstein); "On Jensen's Report: Statement on Race Differences in Intelligence, Occasioned by Arthur Jensen's Article" (Psychologists for Social Action); "How Can We Respond Effectively?" (Howard E. Gruber); and "Selected Bibliographies" of works relating to the nature-nurture issue. [This issue of the IRCD Bulletin also contains the article "Behavior-Genetic Analysis and Its Biosocial Consequences," by Jerry Hirsch (reprinted from the February, 1970 number of "Seminars in Psychiatry") which, however, is not reproduced. Copies of the IRCD Bulletin containing this article may be obtained from the ERIC Information Retrieval Center on the Disadvantaged, Teachers College, Columbia University, New York, N.Y. 10027.] (RJ)

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"Teachability is not a function of hereditability."

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Education, Ethnicity, Genetics and Intelligence

Jensenism: Another Excuse for Failure to Educate

Edmund W. Gordon, Ed.D.

In the midst of a renaissance in the historic struggle of underdeveloped nations and opportunity-denied populations to improve their positions and to assert their rights to equal participation in the societies of the world, we are unfortunately confronted with a reintroduction of old excuses for the failure of our schools to educate large numbers of children. Based upon insufficient trials and with insufficient evidence, we have been offered the conclusion that black and other disadvantaged children have not responded to compensatory education or that compensatory education has failed. We see reopened for debate issues related to the relative contributions of environment and heredity to the development of intelligence, to the possible genetic basis for differential levels of intellectual function in different racial groups, and to the possibility that certain racial groups are inherently limited in their capacity to develop certain cognitive skills. At a time when educators and the society at large are being challenged by a human rights revolution to try harder, the introduction of these arguments could serve to reassure those responsible for facilitating intellectual development that those children we have not served well really are not capable of being developed intellectually. After all, if recent work proves that teachability is hereditable and black children don't inherit it, we certainly cannot and need not teach black children. Clearly, contrary to some colloquial beliefs, ancient and more recent works provide no such proof and as Hirsch has appropriately indicated, **teachability is not a function of hereditability.**

The most recent effort at reintroducing these issues is that of Professor Arthur Jensen in articles which have appeared during the past year in the **Harvard Educational Review.** While Jensen has received some support for his views from a number of sources, he has also come under attack and criticism. Some of his critics have viewed his essay as more political than scholarly, although the extensive review of that portion of the literature which he feels supports his position gives the article the appearance of careful scholarship. Those critics, who view the work as political, have been more critical of the man than of his work and, some would go so far as to deny him a hearing and condemn him for advancing the position. They would also condemn **Harvard Educational Review** for giving him a platform. This is as unfortunate as it is dangerous. Academic freedom, democratic tradition and unbridled scientific inquiry require that he be allowed to pursue, voice and publish his hunches and views, even though he may be wrong as well as out of step with the needs of society. The political criticism of Jensen might better be directed at his judgment and responsibility in preparing and publishing the article in the form in which it appeared and at this time. It is inconceivable that its impact was not anticipated.

An article dealing with these issues at this time should be comprehensive and inclusive of all the evidence. Since more than educational and psychological data and issues are involved, (genetics in particular, anthropology and political economy to a lesser degree), it would have been well for Jensen to have consulted with or had his work reviewed by experts in these fields. Despite his competence in his own field, presumed competence in these related fields may be in error.

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What Jensen Article?

Carol A. Lopate and Edmund W. Gordon, Ed.D.

Approximately fifty percent of the **Harvard Educational Review,** 39(1), is devoted to an article by Arthur R. Jensen, Professor of Educational Psychology and Research Psychologist at the University of California, Berkeley. The article is a revised version of a chapter which appeared in **Disadvantaged Child,** Volume Two, under the title, "The Culturally Disadvantaged and the Heredity-Environment Uncertainty." The Harvard Review article is entitled "How Much Can We Boost IQ and Scholastic Achievement?"

The publication of this paper by the Harvard Review under conditions which forced invited commentaries to appear in the subsequent issue has resulted in considerable controversy in academic and political-social spheres. Not only does Jensen revive the old nature-nurture controversy, he specifically makes assertions and draws conclusions relat-

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The political aspects of this controversy, however, are not my primary concern. There are substantive problems. For educationists, the reintroduction of questions relating to genetics and racial differences as excuses for school wastage is essentially irrelevant. The questions raised simply cannot be answered at this time in this society.

Has compensatory education failed? Those programs and practices which have been offered and called compensatory education have yet to show substantial positive results in terms of sustained and significant changes in the functioning of large numbers of children in the target populations. However, none of the investigators who have undertaken serious studies of compensatory education have reported large scale programs or practices which significantly differ from traditional educational offerings. A few outstanding but small programs have been reported but even these have not exhausted possible innovations. Like ethnic integration in public schools, compensatory education has not failed, it simply has not been actually implemented and evaluated. To assert that disadvantaged children have not responded under this special treatment is like claiming that medication doesn't help when the proper medicine has been unavailable. Such an assertion is fallacious and deceptive. We shall not know whether compensatory education, or for that matter just good education, works until we design appropriate programs, allocate the money required for them, train the people to run them and insure that they are adequately implemented. Any casual observer of the educational scene knows that these steps have not been taken.

Are certain racial groups inherently limited in their intellectual capacities? Margaret Mead and others have edited an excellent book, **Science and the Concept of Race.** This collection of papers, far more important than the Jensen article, but far less widely acclaimed by the mass media, is directed at an examination of the utility and validity of the concept of race in behavior genetics. Positions set forth in two chapters of this book are particularly appropriate to this question:

The nature-nurture controversy, Herbert Birch indicates, is a false issue which has been perpetuated largely through ignorance of advances in scientific research, particularly regarding developmental processes. The confusion between the two concepts "genetic" and "determined" underlies much of the problem. That is, while all aspects of an organism may be thought of as 100 percent genetic, they are not 100 percent determined. Rather, phenotypic expressions are the result of a continuous biochemical and physiological interaction of the gene complex, cytoplasm, internal milieu and external environment throughout the life of the organism.

Developmental influence begins to complicate research in behavioral genetics even before birth through the influence of the maternal environment. Thus even at birth phenotypic expressions do not correlate one-to-one with genotype. Another source of complication arises through differences in rates of maturation and in the patterning of maturation times among separate traits, which may lead to alterations in the patterns of phenotypic expression which do not arise from genetic differences in that specific trait.

Studies in behavioral genetics suffer equally from the fact that behavioral analysis is still at a rudimentary stage. What emerges in most research is the end product of learning a maze, the end score differences in discrimination, or mean differences between groups in intelligence test scores, etc., and there is almost no determination of the specific characteristics of the organism which are involved in the mastery of the presented problem. The classic study by Tyron of the selective breeding of "bright" and "dull" rats illustrates this problem. Analysis of Tyron's study by subsequent investigators showed that what the rats had been selected for was not "intelligence" but responsiveness to visual or nonvisual cues and aspects of temperament. Searle, for example, showed that when visual cues were used, Tyron's "dull" rats were in some circumstances more effective learners than his "bright" strain.

As Birch points out, "If the data of behavioral genetics permit us to draw any conclusions with respect to learning ability it is that learning ability is by no means a unitary trait, and that in different organsms different patterns of responsiveness, of motivation, of emotionality, and of antecedent history contribute substantially to determining which subgroup will learn most effectively under conditions of different instruction and task demand. It appears, therefore, that a sober judgment would lead us to concede that differences in learning achievements, whether measured by intelligence tests or by school achievement in human beings, represent the products of different degrees of goodness of fit between the learner, the task, and, in particular, the instructional mode. Such conclusions have positive rather than pejorative implications for a consideration of differences in learning in style and achievement in human social groupings." [1]

According to Morton Fried, the humanistic intentions of most investigators who have studied intelligence, ability or achievement endowment among different races do not alter the fact that their studies have invariably been based on racial constructs which are destructive and antisocial in addition to being unscientific. In almost all studies, the so-called racial background of individual respondents and respondent populations has been derived in ways that show no resemblance to means used by genetic specialists. In those few cases where any information is given about criteria of assortment, one usually finds that skin color has been the sole or dominant criterion, and that as measured by eye. In other words, the actual genetic background of the subjects is uncontrolled. The classic study by Shuey on **The Testing of Negro Intelligence** illustrates the racist implications of investigations conceived in this mode. In fact, there is as yet no study on a so-called racial sample which adequately links intelligence, potential ability, educability, or even achievement to a specifiable set of genetic coordinates associated with an aggregate larger than a family line or perhaps lineage.

The most useful studies linking race and certain specified socially valued traits make no pretense of dealing with biogenetic race, but openly work with categories of "social race." A case in point is the massive survey, **Equality of Educational Opportunity,** which focused on psychological reactions of being identified and identifying oneself as a Negro in the United States. If race is to be treated as a sociocultural construct, it is important to get the individual's views on his own identification and the identification he applies to others. However, if race is to be treated as a biological construct, the lay individual's views of his own racial identity or that of anyone else is incompetent and immaterial.

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The material from SCIENCE AND THE CONCEPT OF RACE was abstracted for the BULLETIN by Carol A. Lopate.

[1] Herbert Birch, "Boldness and Judgment in Behavioral Genetics," In SCIENCE AND THE CONCEPT OF RACE, (Margaret Mead and others, eds.) New York: Columbia University Press, 1968, pp.49-59.

A Comment on the Jensen Essay

"The improvement of education and other environments is really the only means available to a civilized society for the improvement of the lot and fate of man."

Benjamin S. Bloom, Ph.D.

Arthur Jensen adds heat to the old nature-nuture controversy by speculating on the presumed eugenics consequences of an estimate of the heritability of intelligence. This is an old estimate which many of us have used, but we have used it to determine what could be done with the variance left for the environment. In this article, I will try to further emphasize some of the implications of environmental variance for education and child rearing.

Jensen begins his marathon soliloquy on heredity, race, and eugenics with the attention grabber "Compensatory education has been tried and it apparently has failed." Although he eventually qualifies this, it is only the more persistent reader who finds these qualifications 100 pages later. Jensen indicts compensatory education because he believes it has not produced permanent IQ gains of ten or more points. Compensatory education should be judged in terms of its effects on the students' interests, attitudes, and achievement, not in terms of IQ gains. Some forms of compensatory education and pre-school education have been effective, some have not. Money, effort, and enthusiasm may have been invested in compensatory and early childhood education, but this is not the measure of its value. New approaches to any form of education invariably yield a mixed bag of success, failure, and confusion. It is only as we systematically appraise the results, change our procedures accordingly, and learn from both failure and success, that we can improve our efforts to help our students.

Jensen doubts the likelihood that IQ can be much affected by environmental means other than the intra-uterine environment. Even as unbridled an environmentalist as this writer would prefer to start environmental intervention for educational purposes a bit later than this. Can the environment influence IQ development positively, as well as negatively? Yes, although more is required than a six-week Head Start or three hours a day of a year-long Head Start when the remaining time is spent in an environment which nullifies even these small efforts. The IQ is resistant to easy modification. Perhaps we can learn from the more serious efforts to modify general intelligence in this country as well as abroad. For example, the Israeli experience with Kibbutz child rearing demonstrates that while Jewish children of European origin normally have an average IQ of about 105 under home-rearing conditions, under Kibbutz nursery rearing of 22 hours per day for four or more years, they have a tested IQ of 115. In contrast, Oriental (Mid-eastern) Jewish children have an average IQ of 85 (Jensen's danger point) under home rearing conditions, while under Kibbutz nursery rearing they have an average of 115 (the same as the European children with whom they were matched with respect to education and occupational level of parents and Kibbutz group in which they were reared). But, it is not necessary to provide Kibbutz conditions for our children. The research in this country demonstrates that general intelligence is much affected by home conditions. Some improvement in these conditions can take place, if the parents desire this and are as willing to learn how to do this as they are to learn how to assure their children of a sound physical development or a healthy social and emotional development.

But, Jensen is interested in eugenic solutions. Most of us have a strong distaste for the eugenicist's knife or pill, since we have a strong suspicion that such measures will sooner or later reach us too. Readers of these articles who have an IQ of 115 or higher need not feel too secure. While the eugenicist may start with IQ's of 85 or lower, enthusiasm for this sport must eventually reach higher and higher on the IQ distribution. But, even the eugenicist must pause from time to time for a criterion for his pruning of the family tree. Even he may be puzzled by the large amount of giftedness in the population. For example, if we regard the highest ten percent of the IQ distribution as gifted, then obviously only ten percent of the population can be gifted when we use a single IQ score. However, when we use tests like Thurstone's seven primary mental abilities, we find that 50 percent of the population is gifted by being in the highest decile on one or more of these abilities. If we should increase the number of abilities by a factor of three or more, we are likely to find that most of the population is gifted on one or more of these abilities. The eugenicist has the problem of determining which of these abilities should be sacrificed. The educator's problem is how to develop such abilities, as well as to understand how to make use of varied abilities in the educational process so that all may learn more effectively.

The enthusiastic hereditarian must wonder about what to do with the results of some of the cross-national educational studies. For example, on a recent international mathematics test it was found that most Japanese children exceeded the best of the U.S. children. If U.S. children were graded on Japanese mathematics standards, most of them would fail. Shall we attribute this to heredity? Most of us who were involved in the study were certain that an environmentalist explanation — what happened in the school as well as the home — served to explain why almost all Japanese children would receive the grade of A if marked by U.S. standards.

The psychologist and the geneticist may wish to speculate about how to improve the gene pool — the educator cannot and should not. The educator must be an environmentalist, bridled or unbridled. It is through the environment that he must fashion the educational process. Learning takes place within the child; the educator tries to influence this learning by providing the appropriate environment. If heredity imposes limits, so be it. The educator must work with what is left, whether it be 20 percent of the variance or 50 percent. What he must recognize, however, is that the environment which influences the child is not limited to what happens in the schoolhouse or classroom. Much of the environment has had its effects before the child

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This comment by DR. BENJAMIN BLOOM, Professor of Education at the University of Chicago, was orginally published as a Letter to the Editor of the HARVARD EDUCATIONAL REVIEW, Vol. 39, 1969.

Psychologists Comment on Current IQ Controversy
Heredity versus Environment

The Council of the Society for the Psychological Study of Social Issues issued the following statement on May 2, 1969 in response to Arthur R. Jensen's article, "How Much Can We Boost IQ and Scholastic Achievement?"

"As behavioral scientists, we believe that statements specifying the hereditary components of intelligence are unwarranted by the present state of scientific knowledge. As members of the Council of the Society for the Psychological Study of Social Issues, we believe that such statements may be seriously misinterpreted, particularly in their applications to social policy.

"The evidence of four decades of research on this problem can be readily summarized. There are marked differences in intelligence test scores when one compares a random sample of whites and Negroes. What is equally clear is that little definitive evidence exists that leads to the conclusion that such differences are innate. The evidence points overwhelmingly to the fact that when one compares Negroes and whites of comparable cultural and educational background, differences in intelligence test scores diminish markedly; the more comparable the background, the less the difference. There is no direct evidence that supports the view that there is an innate difference between members of different racial groups.

"We believe that a more accurate understanding of the contribution of heredity to intelligence will be possible only when social conditions for all races are equal and when this situation has existed for several generations. We maintain that the racism and discrimination in our country impose an immeasurable burden upon the black person. Social inequalities deprive large number of black people of social, economic, and educational advantages available to a great majority of the white population. The existing social structures prevent black and white people, even of the same social class, from leading comparable lives. In light of these conditions, it is obvious that no scientific discussion of racial differences can exclude an examination of political, historic, economic, and psychological factors which are inextricably related to racial differences.

"One of our most serious objections to Jensen's article is to his vigorous assertion that compensatory education has apparently failed. The major failure in so-called compensatory education has been in the planning, size, and scope of the program. We maintain that a variety of programs planned to teach specific skills has been effective and that a few well-designed programs, which teach problem-solving and thinking, have also been successful. The results from these programs strongly suggest that continuous and carefully-planned intervention procedures can have a substantially positive influence on the performance of disadvantaged children.

"We point out that a number of Jensen's key assumptions and conclusions are seriously questioned by many psychologists and geneticists.

"The question of the relative contributions of heredity and environment to human development and behavior has a long history of controversy within psychology. Recent research indicates that environmental factors play a role from the moment of the child's conception. The unborn child develops as a result of a complex, little understood, interaction between hereditary and environmental factors; this interaction continues throughout life. To construct questions about complex behavior in terms of heredity **versus** environment is to over-simplify the essence and nature of human development and behavior.

"In an examination of Jensen's data, we find that observed racial differences in intelligence can be attributed to environmental factors. Thus, identical twins reared in different environments can show differences in intelligence test scores which are fully comparable to the differences found between racial groups.

"We must also recognize the limitations of present-day intelligence tests. Largely developed and standardized on white middle class children, these tests tend to be biased against black children to an unknown degree. While IQ tests do predict school achievement, we cannot demonstrate that they are accurate as measures of innate endowment. Any generalizations about the ability of black or white children are very much limited by the nature of existing IQ tests.

"We also draw attention to the fact that the concept of race is most frequently defined 'socially,' by skin color, but that genetic race differences are very difficult to determine. Many of the studies cited by Jensen have employed a social definition of race, rather than the more rigorous genetic definition. Conclusions about the genetic basis for racial differences are obviously dependent on the accuracy of the definition of race employed.

"The Council of the Society for the Psychological Study of Social Issues reaffirms its long-held position of support for open inquiry on all aspects of human behavior. We are concerned with establishing high standards of scientific inquiry and of scientfic responsibility. Included in these standards must be careful interpretation of research findings, with rigorous attention to alternative explanations. In no area of science are these principles more important than in the study of human behavior, where a variety of social factors may have large and far-reaching effects. When research has bearing on social issues and public policy, the scientist must examine the competing explanations for his findings and must exercise the greatest care in his interpretation. Only in this way can he minimize the possibility that others will overgeneralize or misunderstand the social implications of his work."

This statement was signed unanimously by the members of the Council for the Society for the Psychological Study of Social Issues.

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enters school and a good deal of the environment continues outside of the school. Only as the educator recognizes the difficulties he faces, can he adequately help the pupils with whom he works.

But, humans are not cattle to be bred for selected qualities. The improvement of education and other environments is really the only means available to a civilized society for the improvement of the lot and fate of man.

A Flaw in Jensen's Use of Heritability Data

Allan C. Goldstein

Jensen (1969) has argued that compensatory education has failed and that because of this we should look at the evidence for the role of heredity in intellectual performance. In his study, Jensen concludes that intelligence is predominantly inherited, and thus inherited intelligence creates differences in people's abilities to profit from the primarily cognitive teaching methods in the schools.

Jensen is a gifted psychologist. I think he is following the evidence where he thinks it leads him. He is irritated by the promises made by people who have proposed compensatory education, but he is not in favor of dropping compensatory education. Rather, he wants to turn it from the paths that it has followed to date, which depend on cognitive abilities, and to have it rely more on certain lower-level abilities. He is a good man. His ideas have raised fundamental questions about the role of heredity and the nature of the kids who come to school.

There are many places where his psychological evidence is limited or weak. Other papers deal with this. I want to address myself strictly to the genetic question because I am convinced that Jensen has confused two kinds of genetic information, and by doing so has raised an implication of hereditary information which is unfortunately incorrect.

First, I will discuss how genetic data are gathered and analyzed, then will show that, contrary to Jensen's implication, traits that have high loadings on heritability are often highly modifiable. Lastly, I wish to suggest that, in the context of human studies where we are more interested in changing the state of affairs than in philosophizing about them, heritability studies are meaningless.

Heritability Studies

The purpose of biometrical genetics is to take a trait and and to establish the factors that cause it. The most obvious guess is that a trait is affected by both a hereditary factor and an environmental factor. We can write: $P = H + E$ where P is some value for a phenotypic trait (height, weight, musical ability or intelligence), H is a genetic factor and E an environmental factor.

P is the only value we can ordinarily measure. We can, however, get some idea how heredity and environment influence the expression of the trait by observing how the trait varies under different circumstances. For example, we could attempt to measure the intelligence of people in Afghanistan and people in the United States. We would then have two groups of intelligence scores, which we could analyze by means of analysis of variance. One major variable would be country of origin. We would ordinarily think that this would be the only variable. But, in heredity studies, two variables emerge. If we take two genetically pure strains of mice, measure their performance on some task, and then analyze their results, the difference between strains would be a hereditary difference because pure strains are genetically homozygous. But, in experiments like this, the remaining within-group variance is not treated as error, but as variation due to environment. The reason for this is not heredity in this model, but environment. Elaborate formulas are available for expressing the within-and between-group variance as a function of total variance to give estimates of the relative roles of heredity and environment.

The significant thing about experiments like this is that environmental variation is often uncontrolled variance. We know so much more about how to divide up genetic components and so little about dividing up the environmental components, that we forget that in experiments like these, the assumptions about heritability depend upon the normal range of environments of mice, people, or young children. How people would do in various environments is a different matter, and one which would change our conclusions many times. Parsons (1967) concluded similarly:

> "It must be stressed that a heritability so estimated is a characteristic of the actual inbred strains under the environmental conditions prevailing. If the experiment were run under different conditions, or with different strains or both, different values may be obtained." (34)

A look at Jensen's formula will indicate how much emphasis is given to hereditary factors and how little to environmental influences, when it is, of course, exactly the environmental factors that Jensen thinks previous research militates against.

$$V_P = \underbrace{(V_G + V_{AM}) + V_D + Vi}_{V_H} + \underbrace{V_E + 2\,Cov_{HE} + V_I}_{V_E} + V_e$$

where V_P is the variance of the phenotypic traits, V_G the additive genic variance, V_{AM} the variance due to assortative mating, V_D the derivation variance due to dominance effects, Vi variance due to interaction of certain alleic sites, V_E the environmental variance, Cov_{HE} the covariation of heredity and environment, V_I the statistical interaction of genetic and environmental factors, and V_e the error variance.

In the first term, which is the amount due to heredity, four sources of variation are stipulated: a genic one (genes acting independently, therefore additively), one due to gene interaction, one due to dominance, and one due to assortative mating. In the second term, there is only one factor attributable to environment (V_E). The other two terms are interactions of this term with some other terms on the heredity side of the equation. The term describing the environment component of variance is not due to experimentally-manipulated parts of the environment, but environments as we find them. Yet, we are being asked to conclude about all environments, when we are asked to decide how heredity is more important than schemes of compensatory education.

I should not like to disparage this line of investigation when appropriate experiments can be done. When we wonder whether it is better to grow two kinds of potatoes, or one kind of potato and pray for rain, answers that

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come from this kind of research are helpful. Indeed, if we abstractly wonder whether the kinds of environments in which we raise social, economic, religious or racial groups do much to alter the performances of people they affiliate with, this may have some value. But when we use these data to bolster our feeling that heritability acts, therefore environment does not, we are drawing conclusions that the model is not prepared to deal with.

Is there any evidence that taits that have high heritability are modifiable? Does heredity mean that a child of a certain hereditary background who does poorly on a trait with a high heritability component is doomed to continue to do poorly? No! Let us see why.

Traits With High Heritability Are Often Modifiable

Let us momentarily accept maze learning ability in animals as an analog for human intelligence though it is far from being a very direct one. Tryon (1940, 1942) at the University of California investigated the inheritance of maze-learning ability in laboratory rats. Over a period of 11 years he raised 18 generations of rats. The original sample consisted of 142 animals. He tested them in a fully automatic maze, found a normal distribution of errors on his 17-blind maze, and bred males and females that solved the problem with relatively few errors and other males and females that made a great number of errors. He found that by selectively inbreeding the brights and the dulls over several generations he was able to produce offspring who solved the maze very well or very poorly, depending upon the maze performance of their parents. All we needed to know about them was who their parents were and we could predict without error whether they would do well or poorly in the maze. They "inherited" maze learning ability. As a matter of fact, after seven generations, there was little overlap between the two populations and this was maintained through an additional 11 generations. This is clear and strong evidence of strict inheritability of maze learning ability. In a different task, Thompson (1954) found that a totally different kind of maze-learning ability was similarly inheritable in rats. There was no overlap after the fourth generation.

During this period of time, however, under the stimulus of Hebb's neuropsychological theory, his students and other people wondered whether the immature nervous system required some kind of environmental stimulation in order to develop its full adult potential. Three early papers, appearing in 1952 were only the beginning of a series showing that early experience improved, and impoverished experience diminished the ability of adult laboratory rats to solve mazes. (Bingham & Griffiths, 1952; Hymovitch, 1952; Forgays & Forgays, 1952). Each of these people showed that, in a random group of rats, early exposure to a perceptually-rich environment improved the ability of these animals to solve adult mazes later on.

Hebb at a symposium in the 1950's commented on these data along with Tryon's and Thompson's heritability data by suggesting that maze-learning ability is 100 percent inherited and 100 percent environmental. Witty and apt—but, phenotypic variation is not 200 percent of something. but some division of heredity and environment.

The crucial experiment is, of course, to find out the relative contributions of both heredity and environment. By analogy to Jensen's reasoning, the demonstration of heritability in maze learning should counter any substantial effect of environment in influencing the performance. Jensen specifically believes that intelligence in people is a threshold variable, that is, a poor environment can certainly interfere with a child's ability to do well in school and on intelligence tests, but a good environment cannot improve a bad heredity.

The data of Cooper & Zubek (1958) show the opposite. When they raised hereditarily dull animals in an enriched environment, the rats did as well as hereditarily bright animals raised under normal circumstances. There was, in addition, the kind of threshold effect postulated by Jensen: bright animals in an impoverished environment did as poorly as dull animals in a normal environment, showing that something about these animal data accords with what Jensen suspects, so greater weight might also be given to the data which are exactly the opposite of what Jensen would have us believe heritability means.

These data (and I have selected only data bearing directly on intellectual performance in animals) show us that even when we have the kind of heritability data that is experimentally obtained (not as Jensen's, an *a posteriori* model), we see that when we find an appropriate kind of environmental alteration, the "genetic" potentialities of animal strains can be modified totally.

We can quote Hebb & Thompson (1954) on an important implication of these data. They say:

> "Experimental studies have shown clearly that when the postnatal environment is held constant, the level of learning capacity and problem-solving in the rat is innately determined . . . that is, one can breed rats that will be brighter or duller when brought up in the same environment. But, they show equally clearly that when heredity is held constant between experimental groups by the split-litter technique, infant experience determines the level of adult problem-solving. For the rat, then, adult intelligence depends both on heredity and on the stimulating action of the postnatal environment." (533)

These word, written before the Cooper & Zubek, strengthen their conclusions.

One also does well to remember Eysenck's remark in this connection: Heredity, he said, becomes increasingly more important when the environment is made more and more uniform. When we measure heritability in an educational world uniform for all children or in a world where we have not found the right educational experiences, we have arbitrarily set up the conditions for maximizing the variance assigned to what we call heredity in an analysis of variance. We have not said anything about its role when we have constructively altered that environment. Cooper & Zubek's data are excellent illustrations of this.

There is a different line of evidence on this question. Some of the strongest data favoring heritability of intelligence in people are the correlations among intelligence test scores of varying genetic closeness. Jensen summarizes the data as follows:

Children reared apart	—0.01
Parents and their children	+0.50
Siblings reared together	+0.55
Dizygotic twins, different sex	+0.49
Dizygotic twins, same sex	+0.56
Monozygotic twins, reared together	+0.87
Monozygotic twins, reared apart	+0.75

(Adapted from Table 2)

(Continued on page 7)

Goldstein (Continued from page 6)

Children reared apart are totally unrelated and their scores correlate close to zero; they are statistically, as well as genetically, independent. Parents and their children correlate about +0.50 as do children in the same family, nonidentical twins of the same or opposite sex. When we turn to identical twins (the closest genetic similarity seen in people, the correlation is greatly elevated +0.87. When identical twins have been raised apart the correlation is still +0.75. In studies on heritability, the standard comparison has always been between identical and fraternal twins; the latter have only a general familial genetic similarity. Here the correlation is about +0.85 versus +0.50. Clearly there is a greater relationship between identical than fraternal twins. Clearly we have a strong heritability factor.

Data exist for other traits. Height and weight show similar results supporting our generally-held assumption that hereditary familial factors are responsible for them.

But now let us mention another kind of data which shows hereditary effects: concordance of disease in twins.

	Monozygotic Twins	Dizygotic Twins
Diabetes mellitus	84%	37%
Paralytic poliomyelitis	36%	6%
Rickets	88%	22%
Tuberculosis	74%	28%

(Adapted from C. Stern, **Principles of Human Genetics,** 1960, Fig. 225)

Diabetes mellitus has a much higher incidence among identical twins than fraternal. What if someone had convinced us that these heritability data made it useless to search for a hormone-like insulin that could completely control the disease and so allow millions of diabetics to live normal lives? Would that have been wise scholarship? What if someone had used the data on paralytic poliomyelitis to convince us to stop the search for vaccines which, we now know, have spared the lives of hundreds of children every year? Would that have been wise scholarship? What if the astonishing data on rickets, a classic hereditary disease that "ran in families" somehow discouraged the search into vitamin mechanisms so that vitamin D was never made available, small amounts of which dumped into gallons of milk have made medical curiosities of rachitic children? Would it have been wise scholarship for thousands of children to walk with bowed legs? Tuberculosis: what is the price we would be willing to have paid not to have found tuberculostatic drugs because the search was not worthwhile because of strong heritability data?

Let us look at one final set of data that meet all of Jensen's criteria: they are derived from people, they are clearly inherited, and they are based on measured psychometric intelligence.

Phenylketonuria is a crippling disease, produced by the biochemical inability of the body to metabolize the essential amino acid phenylalanine to tyrosine. It is extremely rare, afflicting about one child born in 10,000 to 20,000. Yet 53 percent of the families that have one phenylketonuric child have more than one, showing a very high familial incidence. Genetic studies have shown that the disease is due to a single abnormal autosomal recessive gene. If one were to do heritability studies similar to those reported by Jensen, there would be a very high proportion of phenotypic variance attributed to heredity and little to environment; and thus we should have to think about how to train these children by drawing upon their limited capacities. These are limited indeed. Eighty-five percent of the children have I.Q.'s under 50.

Research within the last 15 years has shown that this disease can be controlled by recognizing it early and giving these children diets low in phenylalanine. With this treatment, these children grow up with I.Q. scores near, or within, the normal range. Exactly how these children improve is a matter of future research, and there is not complete agreement on these data (Birch & Tizard, 1967; Fuller & Shuman, 1969). But even if dietary treatment is not the full answer, at least the search has begun. What is an indisputable fact in this instance, as in the other instances mentioned, is the shortcoming of Jensen's statistical arguments.

Heritability factors are present in every single human trait we can imagine that is grounded in biological functioning. We know that psychometric intelligence and the intelligence of Piaget are such. To the extent that we test people in a uniform environment, hereditary contributions to these differences will be maximized. The presence of hereditary differences tells us absolutely nothing about what we shall discover tomorrow concerning some environmental trick that will allow us to completely remove heritability as a major factor in accounting for phenotypic variation. It is misleading, and it is cruel in the climate prevailing now to naively suggest that the presence of high heritability scores tells us about a limited role for environmental factors.

What Then Is Heritability?

I want to return to Jensen's formula, for the confusion that this discussion produces lies in his discussion of one term in that formula. This is what he says about the V_I term:

> "The **interaction** of genetic and environmental factors (V_I must be clearly distinguished from the **covariance** of heredity and environment. There is considerable confusion concerning the meaning of interaction in much of the literature on heredity and intelligence. It is claimed, for example, that nothing can be said about the relative importance of heredity and environment because intelligence is the result of the 'interaction' of these influences and therefore their independent effects cannot be estimated. This is simply false. The proportion of the population variance due to genetic X environment interaction is conceptually and empirically separable from other variance components, and its independent contribution to the total variance can be known. Those who call themselves 'interactionists," with the conviction that they have thereby either solved or risen above the whole issue of the relative contributions of heredity and environment to individual differences in intelligence, are apparently unaware that the **preponderance of evidence indicates that the interaction variance, V_I, is the smallest component of the total phenotypic variance of intelligence.**" (Author's, not Jensen's, emphasis.)

Jensen is right on this last point. On the next to last point in this paragraph, he is wrong. Bound up in one tradition of genetic analysis — the tradition of trying to interpret the bad information we have from the past, instead of experimentally asking questions about the future — Jensen fails to see that, when this interaction term is insignificant, this is an admission of ignorance — a statement that up to the present we know only those environ-

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Jensen Article (Continued from page 1)

ing to racial differences in intellectual ability. His position supports views attributing a higher level of intelligence to whites than that attributed to blacks and implying a different quality of educational experience for each group. In the current period of intense concern for equalizing opportunity and status for minority groups, this article has had an incendiary impact and, whether or not it was intended as a political document, its theoretical implications and related unresolved research problems have become the focus of political as well as academic debate.

In this issue of the **IRCD Bulletin,** we try to shed some light on both aspects of that debate. For our readers who have not seen the Jensen article, a synopsis of the original article which appeared in the **Harvard Educational Review** follows.

A Synopsis

"How Much Can We Boost IQ and Scholastic Achievement?" begins with Jensen's conclusion that, as measured by changes in IQ scores and scholastic achievement, compensatory education programs in the United States have failed. Such programs, according to Jensen, are based on the "average child" concept, which assumes that, except for a few rare children born with neurological defects, children are basically very much alike in their mental development and capabilities and that differences in performance are due "rather to superficial differences in childrens' upbringing at home, their preschool and out-of-school experiences, motivations and interests, and the educational influences of their family background." In Jensen's view, the alleged failure of existing patterns of compensatory education programs to produce significant and/or lasting changes in IQ or scholastic achievement calls for a re-examination of the nature of intelligence.

What is intelliegnce? The simplest definition is an operational one: it is what such intelligence tests as the Metric Scale of Intelligence, the Stanford Binet or the Wechsler measure. Jensen is careful to note that he does not regard intelligence as completely synonomous with "mental ability," which refers to the totality of a person's mental abilities. However, he maintains that, since these tests predict various measures of scholastic achievement with an average coefficient of about .5 to .6 and retain approximately the same reliability with longitudinal data, intelligence tests may be considered a useful predictor of intellectual and academic achievement. Moreover, Jensen provides examples to illustrate the intercorrelation between numerous, quite varied, psychological tests of "mental ability" and concludes that a common factor (called by Spearman "general intelligence" or "G") accounts for the intercorrelations among these tests. According to Jensen, "The common feature of all such intercorrelated tests seems to be their requirement of some form of 'reasoning' on the part of the subject—some active, but usually covert, transformation or manipulation of the 'input' (the problem) in order to arrive at the 'output' (answer)." Although this G factor may be analyzed into more specific ability components, Jensen believes that the components are not manifested separately or independently in the general functioning of intelligence.

Having identified himself with this position relative to the nature of intelligence, Jensen reports independently arrived at intercorrelations of .81 to .91 between socioeconomic status, as indicated by the Barr scale of "intelligence demands" of an occupation, and ratings of occupational prestige based on National Opinion Research Center scale. Although there is a considerable dispersion of IQ's within occupations, with the spread increasing as one moves down the scale, Jensen cites evidence that intelligence (if only indirectly through education) is related to both occupational choice and job performance within occupations.

These data are used to raise questions concerning the relative forces of heredity and environment in forming intelligence and the degree to which measured intelligence is mutable or fixed throughout the life span of an individual. Jensen refers to Hunt's book, **Intelligence and Experience,** which tends to support a counter argument to the notion of fixed intelligence, but identifies himself with the less optimistic view of the mutability of intelligence, as reported in Bloom's book, **The Stability of Human Characteristics.** Bloom argues that IQ, like other developmental characteristics, is quite variable in early life before the age of five but, becomes increasingly stable as the individual matures.

Jensen reports the distribution of IQ scores throughout several populations and finds that, except for certain systematic departures, intelligence test scores form a normal distribution as represented by the bell-shaped curve. Exceptions at the extreme lower end of the continuum, he finds, are generally linked to genetic abnormalities. Particularly among children of higher social-economic status (SES) parents, almost none have subnormal IQ scores unless they also have neurological abnormalities, such as epilepsy, sensory defects, atypical EEG readings and chromosomal or biochemical disorders.

From this and other evidence cited, Jensen suggests that the hereditability of intelligence is quite high; that is, that genetic factors are far more important than environmental forces in producing IQ differences among individuals as well as between populations. His evidence includes studies of selective breeding for intelligence in rats and other animals, investigations of the effect of the chromosomal anomaly called Turner's syndrome on specific forms of intellectual functioning, studies of mental retardation, and research concerning the high degree of assortive mating in our society and its relationship to significant differentials in the intelligence of offspring.

Jensen sees as an important problem for investigation the determination of the portion of variance in intellectual function which can be attributed to heredity and that which can be attributed to environment. In recent years, investigators have assumed an interaction between heredity and environment in forming human characteristics, but have felt that, with the exception of quite rudimentary characteristics, there has been insufficient data for the task of separating out genotypes from phenotypes. In Jensen's view, however, the proportion of the population variance due to genetic factors as opposed to environmental interaction in forming even such a complex characteristic as intelligence is "conceptually and empirically separable from other variance components, and its independent contribution to the total variance can be known." Thus Jensen attempts to determine the proportion of phenotypic variance which is due to genotypic variance. A comprehensive review of studies of unrelated persons reared together and apart, collateral relations including siblings, dizygotic and monozygotic twins) reared together and apart, and direct-line relations (grandparent and grandchild, and parent and child) provide his main data. From these data he concludes that approximately twenty percent of the variance in IQ is attributable to environmental influences (E) and 80 percent to heredity (H).

Jensen reviews literature which reports that intelligence can be boosted as much as 20 or 30 points if the environ-

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On Jensen's Report

STATEMENT ON RACE DIFFERENCES IN INTELLIGENCE

Occasioned by Arthur Jensen's Article
"How Much Can We Boost IQ
and Scholastic Achievement?"
Harvard Educational Review, 1969, 39, 1-123

The suggestion that there is a scientific basis for drawing conclusions about race differences in intelligence is based on the following premises:

1. That intelligence tests provide a reliable measure of a scientifically understood entity called "intelligence."
2. That there has ever been a single black child in this country who has not suffered from some form of racial oppression.
3. That there is a body of accurate information based on the administration of intelligence tests to black and white children under comparable circumstances.
4. That intelligence is inherited as a fixed trait.
5. That the science of genetics provides a basis for the definition of intelligence in terms of innate and acquired components.
6. That hunger and malnutrition, prenatally and during early childhood, do not seriously impair and debilitate intellectual performance.

All of the above premises are either obviously false or seriously questioned by many competent scientists working on these problems.

In fact, there is little agreement as to what intelligence tests measure: the recent explosion of knowledge about intellectual development is not at all reflected in intelligence tests now in use; there is positive evidence that scores on intelligence tests are not closely related to creativity. In fact, class, race, and language differences between tester and testee always put the poor child, the black child, and the Spanish-speaking child at a disadvantage. In fact, the competitive attitudes engendering 'good' test performance are widespread in white communities and atypical in many minority groups.

The only scientifically established race difference in this country is the oppression of black and other minority groups by the white majority: their condemnation to lives of poverty and fear, ill-health and rotten education.

It would be unfortunate if a speculative article were used as the basis for the formation of social policy. There is a natural tendency for supporters of the status quo to seize upon suggestions that the human condition is unalterable. Thus, those opposing reductions in military budgets welcome suggestions that something in human nature makes war inevitable; those opposing increases in expenditures for social welfare welcome suggestions that there is something that makes poverty inevitable. Similarly, those who profit directly or indirectly from the inferior status of black Americans will welcome the suggestion that their own privileged social position is biologically determined.

The task facing American scientists is not to make excuses for the failures of our society, but to seek out creative ways of rectifying the social evils that afflict us. Social policy must be based on the goals of treating each person as an individual with his own needs, and on the right of each person to all the opportunities and social services necessary to permit his maximum personal growth and dignity.

We call upon our colleagues in all disciplines to join us in a vigorous effort to use our scientific knowledge for the elimination of the evils of a racist society, and the construction of a truly egalitarian nation.

This statement was prepared by PSYCHOLOGISTS FOR SOCIAL ACTION, and circulated at American Psychological Association, Eastern Psychological Association, and other professional meetings. By December, 1969, it had been signed by over 400 psychologists, both PSA members and nonmembers. This article is reprinted from the December, 1969 issue of SOCIAL ACTION.

Jensen Article (Continued from page 8)

ment is changed from one of extreme deprivation to an enriched one, but maintains that there are no studies showing that children can be given permanently superior IQ's by means of environmental manipulation. Moreover, he cites evidence to show that with age children's IQ's increasingly resemble their parents' intelligence, whether they are reared by them or not. He concludes that such changes are unlikely to be due to environmental rather than genetic factors.

Granting that a proportion of an individual's or a population's intelligence is influenced by environmental factors, Jensen discusses which of these factors are most likely to be important in influencing intelligence. His survey of the research leads him to conclude that reproductive casualties, intrauterine conditions, birth weight, prematurity and birth order, which are related to conditions of pregnancy and in turn to socio-economic level, correlate significantly with IQ. However, Jensen hypothesizes that even these conditions may be genetically related, since they appear with greater frequency within certain populations.

Finally, Jensen discusses the issue of race and intelligence. Holding SES constant, he reports that Negroes test about 11 IQ points below the average for the white population. Since genetic pools caused by intermarriage are said to give rise to a number of phenotypic characteristics having high hereditability, he questions why differences in intelligence cannot also be attributed to "breeding populations, or race," particularly since,"No one has yet produced any evidence based on a properly controlled study to show that representative samples of Negro and white children can be equalized in intellectual ability through statistical control of environment and education."

Jensen states that studies on the effects of compensatory education programs have generally been poorly conducted in terms of control groups, descriptions of treatment given, measurement of specific skills gained and duration of these gains. Reviewing the effects of preschool projects, where investigations have been most numerous and complete, and where presumably the most change can be effected, he concludes that such programs can undoubtedly have a "hothouse" effect, "forcing an early bloom which is no different from a normal bloom," but he questions whether they can act "like a fertilizer, producing bigger and better yields."

Extrapolating from studies using a number of the more traditional as well as the newer and less culture-bound intelligence tests, Jensen analyzes intelligence into two kinds of abilities: associative learning ability, which he calls "level I" and cognitive or conceptual learning and problem-solving ability, which he calls "level II." According to his analysis, lower and middle class elementary school children perform equally well on level I type tasks, although they may differ 15-20 points on level II type tasks. It is these level II type tasks which show up in the generalized IQ factor G.

Thus he concludes:

> "I am reasonably convinced that all the basic scholastic skills can be learned by children with normal

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How Can We Respond Effectively?

Howard E. Gruber

This article is reprinted from the December, 1969 issue of SOCIAL ACTION.

Under the title, "How much can we boost IQ and Scholastic Achievement?" Arthur Jensen of the University of California, Berkeley, recently published a seemingly well-documented argument that black people are genetically inferior to whites. His article was widely and enthusiastically reported in newspapers and magazines, and it has already been used as evidence in support of legislative and legal attacks on desegregated education.

Responses to Jensen, some of them at a very high level of technical competence, have not been wanting. The **Harvard Educational Review,** which published Jensen's article in its first 1969 issue, carried numerous critiques in the two subsequent issues. Although couched in restrained academese, geneticist James Crow's reply does much to dispel the notion that Jensen's argument is based on a firm conceptual footing in the theory of population genetics; but Crow is so polite that the hasty reader might almost gain the impression that Crow agrees with Jensen! Perhaps the most telling critique is the paper by Richard Light and Paul Smith, "Social Allocation Models of Intelligence," in which they show that "a large difference between black and white mean IQs may be explained not by the hypothesis of genetic differences between races, but rather by the non-genetic differences in allocation of blacks and whites to different environments..." Although their paper is complex, and dependent on computer analysis of various models, it is very clearly written and intelligible to a reader of only modest statistical and genetical attainments.

As for public pronouncements, Psychologists for Social Action circulated a critical statement at the 1969 EPA meeting in Philadelphia, and subsequently, which has been signed by thousands of psychologists. The Society for the Psychological Study of Social Issues published a statement unanimously endorsed by its governing Council, and reprinted in the **Journal of Social Issues,** summer 1969. See also the November 1969 **American Psychologist.**

Belatedly, and in its back pages, the **Harvard Educational Review** published a statement by the Black Student Union of the Harvard Graduate School of Education pointing out that the question of racial equality is "of course, far more political than scientific," and criticizing the editors for failing to consult Black students or faculty before publishing the Jensen article and for failing to publish rejoinders in the same issue. It was the latter omission that gave the segregationists an easy opportunity to make racist hay out of academic manure.

Then, the sop — perhaps Negroes have other virtues. In special schools, they can develop their own racial potential. Incidentally, Jensen carefully avoids nasty words like 'inferior' — but the veil is very thin.

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The structure of his argument, then, contains one model of social research worth noticing. He begins with a social experiment and its evaluation; this leads him to a weighty scientific question which he then treats in some detail; he then feeds the results of this treatment back into the domain of practical application. Jensen's argument was useful to the racists, not only because he marshalled the case for race differences, but because he embedded the case in a broad social matrix, a "closed loop" — from application to evaluation to research, and back to application. To put it another way, he is not only "relevant," he is pointed.

We psychologists who are dedicated to the eradication of racism can learn something from the unfortunate success of Jensen's efforts. His article had powerful effects because he devoted considerable effort to the synthesis of a technical analysis with a social policy. Consider the structure of his argument: Compensatory education has failed. This raises the question, can it ever succeed? If Negroes are genetically inferior, social programs aimed at equality are doomed. Then, the central thesis, leading to his conclusion that Negroes are genetically inferior in intelligence.

The full answer to Jensen will have to be equally attentive to closing the loop in the cycle of protest-criticism-research-invention-political action application. There is need in this work for the talents and energies of psychologists of many kinds. It would be instructive for the reader, for example, to take half an hour to imagine the concerted effort to produce and disseminate a pamphlet on race and intelligence: what should go into the pamphlet? is any original research needed? who should write it? how would the printing be funded? granted some limits to our resources, to whom would it be best to distribute it? what special point would there be in a document produced by members of PSA? should it deal with racism per se, or only with the nature-nurture question as it applies to race and intelligence? should it deal with the rather limited experiments in compensatory education which have been conducted to date? if so, in a spirit of criticizing their limitations or extolling their sometime successes? if the pamphlet is designed for the members of the boards of education in 30,000 American school districts, for instance, should it go into the need for radical social reform, or take a more limited perspective? (½-hour later) Now that you have done this thought-experiment, do you want to help with the real thing? If so, let us know.

Jensen Article (Continued from page 9)

> level I learning ability, provided the instructional techniques do not make G (i.e. Level II) the **sine qua non** of being able to learn. Educational researchers must discover and devise teaching methods that capitalize on existing abilities for the acquisition of those basic skills which students will need in order to get good jobs when they leave school. I believe there will be greater rewards for all concerned if we further explore different types of abilities and modes of learning, and seek to discover how these various abilities can serve the aims of education. This seems more promising than acting as though only one pattern of abilities, emphasizing G, can succeed educationally, and therefore trying to inculcate this one ability pattern in all children."

Gordon (Continued from page 2)

A scientific definition of race and of specifically designated racial groups should be based on specifiable metrical and morphological features capable of intersubjective identication. Moreover, given the probability of increased numbers of individuals having internodal constellations of genotypes, correct racial typing is more and more difficult and demands full-scale attempts to control the geneological histories of all subjects. As a result of the proficient training of the new physical anthropologist, we may expect studies of small populations, well controlled for genetic information. But these will be studies of populations, not races. In fact, as Fried says, "scientists carrying out **bona fide** studies of populations will have the further obligation to present the results in such a fashion as to make difficult, if not impossible, their pseudoscientific application to race. Should such usage be attempted, the scientists who authored the original study must immediately reject and disown the false application."[2]

What are the relative contributions of hereditary and environmental factors to the development of intelligence? This question simply cannot be answered. This in part accounts for the fact that it continues to be debated. The fact is that the technology of human genetic research does not permit definitive study of the genetic constitution of human organisms. We have only within the past year isolated a single gene and that in a bacterium. If such technology were available, our attitudes toward research utilizing human subjects would not permit it. They would at least preclude the inclusion of high status groups in experimental, comparative studies. In addition, the economic, political and military commitments of the society obviously do not permit the kinds of experimentation with controlled and improved environmental conditions necessary to the conduct of such studies.

Even if the question could be answered and even if it were definitively determined that a specific portion or aspect of human behavior and potential were fixed by heredity, as a humanist and as an educator, I would still have the commitment to, and the responsibility for, expanding and optimizing the influence of environmental interactions. That is what directed learning or education is all about. Educators cannot manipulate genes, we can control experience and our environments.

If and when we are able to speak intelligently about the portion of intellectual function attributable to heredity, it will only be under specific interactions or conditions. When we talk about intelligence we are talking about phenotype, and phenotype by definition is a function of environmental interactions with genotype. It is my guess that when and if we are able to separate genotype in human behavioral development, its function will ony be determinable in relation to, or as it is expressed through, phenotype. And in that relation its function will be determinable only to the extent that the **interaction** is specified.

The rather pessimistic view of the plasticity of human potential in selected populations which is advanced by some investigators is in part a function of their limited view of potential significance of interaction variance. As long as they view environmental interactions in "normal" or traditional terms they severely limit their perspectives and hopelessly bias their results. They seem to dismiss the possibility that interactions can be, may be, and in some instances have been made significant in specified directions. Goldstein has noted in his article, "A Flaw in Jensen's Use of Heritability Data," which appears on page seven:

> "The discovery of insulin, the isolation of Vitamin D, the production of tubercolostatic drugs, the uncovering and control of phenylketonuria are all those exceptional environmental changes which will make this interaction term significant. They indicate that environments everywhere are not merely supportive of hereditary potentialities but can at times reverse deleterious effects. **The great achievements of mankind lie in making that interaction term significant.** Indeed, it could almost be a maxim for schools of education, psychology, public health, medicine: **"Make that interaction significant."**

My argument with those who hold the more narrow view is that, despite their claim to interest in the advancement of science, few of them support the kinds and magnitude of changes in social, economic, and political conditions which could make the interaction component more significant and their own research more possible. I cannot accept this stance since it is supportive of the status quo, it means business as usual, it means limited opportunity for black and poor people—it means that we invest too little effort in trying to make the interaction significant, while the majority tries to fix the blame on the victims or on nature for differences, underdevelopment, or school failures which are largely imposed on lower-status persons by man's indifference to, or abuse of, his fellow man.

It is in this context that the controversy does have relevance. The manner in which investigators or practitioners approach the understanding of the organization of behavior or the modification of behavioral organization is greatly influenced by the theoretical position held with respect to the genesis of patterned behavior. Problems raised for investigation, the research design chosen, as well as the phenomena which are observed, generally reflect the theoretical bias of the investigator. The choice of goals and practices to be utilized in approaches to behavioral change are usually determined by the practitioner's view of the mechanisms underlying behavioral organization.

Theories of behavior may be divided between those which posit an essentially projective view and those which posit an essentially reflectional or interactive view of the mechanisms underlying behavioral organization. In the projective view predetermined, intrinsic patterns are thought to be released by stimulation and projected onto the environment where their specific form is facilitated or inhibited. Among persons adhering to this position, emphasis is given to hypotheses concerning the existence in the organism of intrinsic drive states which exist prior to, and independently of, life experiences and which are the basic forces in the determination of behavior. Certain behavioral patterns are seen as performed, stored, and waiting for the proper time and condition for emergence. Capacities and traits are seen as determined by these intrinsic factors which can only be **modified somewhat** by the environment. The fundamental character of patterned behavior is seen an genetically established and bound. Environmental forces are considered to **influence** the organization of behavior by determining: 1) the directions taken by the primary energies and drives;

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[2]Morton H. Fried, "The Need to End the Pseudoscientific Investigation of Race," In SCIENCE AND THE CONCEPT OF RACE, Margaret Mead and others, eds.) New York: Columbia University Press, 1968, pp.122-131.

Gordon (Continued from page 11)

2) the environmental objects to which they become attached; and 3) the specific time and form in which they will emerge.

On the other hand, the interactionist or reflectionist position holds that all organized patterned behaviors are reflections of the interaction between the organism and its invironment. Environmental interaction is seen as the crucial determinant and molder of patterned organismic function. Temporal and situational phenomena are not **releaser,** but **causative** and **mediating** agents. Behavioral characteristics, traits, species typical behaviors — all organized patterned behavior — are seen to exist only as a result of sensory input flowing from the interaction between the organism and the environment. Behavior **potentials** are said to be genetically seeded in the sense that the organism includes structural responsivity which is determined by interactions between organism and environment with the nature of these interactions being critical for organized behavior.

Now, what is the relationship of this controversy to directed learning? When behavior theory is used to guide practice in directed learning, the differences in the two positions are manifested in the goals, the design, and the management of learning experiences. (Directed learning is used here as an inclusive term to cover education, counseling and guidance.)

The dominance of the projective view in directed learning has been reflected in:

a. A laissez-faire or neglectful attitude toward the training and development of intelligence

b. A monitoring as opposed to a stimulating approach to academic and social readiness and personality development

c. An exaggerated emphasis on the predictive value of the classification and quantification of psychological appraisal data and the neglect of qualitative appraisal data as a basis for planning, prescription and intervention

d. Distortion of aspiration and expectation levels based upon unjustified ceilings on potentials for human development and adaptation

e. Placement of the burden of proof: 1) on the examinee rather than on the appraiser or appraisal method; 2) on the learner rather than on the teacher or teaching method; and 3) on the counselee rather than on the counselor or counseling method

f. Emphasis on adjustment to or acceptance of assumed realities rather than on modification of the environment and the individual's interaction with same

g. Overemphasis on selection and placement with an underemphasis on the nurturing of interests and aptitudes and the development and training of capacities and skills

The constricting and limiting influence of this theoretical bias on education in general and compensatory education in particular led me in 1965 to prepare a statement for the American Orthopsychiatric Association. That statement is reprinted here because of its prophetic accuracy and relevance to the current controversy.

Help for the Disadvantaged?[3]

All of us who identify with the scientific-humanitarian traditions which have characterized the Orthopsychiatric Association must derive some sense of achievement from the growing attention being directed at problems of the disadvantaged. Our Association only recently has lost its uniqueness in devoting serious attention to the implications of cultural difference and/or disadvantaged status for service in the helping professions. Indeed, it appears that the Association, which for so long stood almost alone in advocating the application of concepts of differential psychology, public health, compensatory education and aggressive casework to mental health, social work and educational services, now has many companions. In addition, isolated and pilot programs which for years have struggled to stay alive suddenly now face the prospects of abundant support and massive replication. Private foundations and government stand ready as never before to pour money and human resources into work directed at the disadvantaged.

It is tempting to anticipate that the current outbreak of enthusiasm will produce results consistent with the quantity of time, energy, money and concern being expended. However, in dealing with problems for which solutions are based upon significant social and scientific advances, popularity and productivity do not necessarily go hand in hand. In the present situation there is grave danger that work with the unfortunate may, unfortunately, become a fad. So great is the danger that it may not be out of place to suggest that the appropriate attitude at this time for those truly concerned with the long-range goal of significantly improving the life chances of disadvantaged populations, is one of restraint and considered action. It is obviously not the quantity of effort that will solve the problems here involved. Work of high quality which more correctly reflects scientific and social reality finally will give this result.

Having recently reviewed much of the research and most of the current programs concerned with the disadvantaged, I am impressed by the pitifully small though growing body of knowledge available as a guide to work in this area. The paucity of serious research attention to these problems has left us with little hard data, many impressions and a few firm leads. What is distressing, however, is the slight representation of even this research in the rapidly proliferating programs. Much of what is being done for and to the disadvantaged seems to be guided by the conviction that what is needed is more of those things we feel we know how to do. Despite the fact that much of our knowledge and techniques of behavioral change have proved to be of dubious value in our work with more advantaged populations, these same procedures and services now are being poured into the new programs. Although service to the disadvantaged has become popular, there remains a serious lack of basic research on the developmental needs of such children as well as on the applicability of specific techniques of behavioral change to their directed development.

It is not intended to suggest that the extension of known techniques to these previously neglected populations is entirely negative. Humanitarian concern calls for the use of all possible resources to relieve human suffering. What is suggested is that there may be vast differences between what we feel we know how to do and that which must be done. To settle for what we "know" while we ignore new concepts and the exploration of new leads renders us less humanitarian, less scientific and less professional. Unfortunately, our society has permitted us to place the burden of proof of the worth of our services on the beneficiaries of these services rather than on the professional worker or the system in which he functions. This has permitted us to ignore or rationalize our failures. If real progress is to be made, we as professionals must assume greater responsibility for the success of our work, recognizing that it is

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Gordon (Continued from page 12)

our role to better understand these problems and to design techniques and measures more appropriate to their solution. It must be clear to all of us that more counseling is not going to solve the problems of a population we have defined as nonverbal. Reading texts in technicolor are not going to solve the reading problems of youngsters who we claim are deficient in symbolic representational skills. Reduced demand curricula and work study programs are not going to advance the conceptual development of youth whose conditions of life may have produced differential patterns of intellectual function which are so frequently interpreted as evidence of mental retardation rather than as challenges to improved teaching. Occupational information and aspirational exhortation are not going to provide motivation for youth who have yet to see employment opportunities, employed models with whom they can identify and accessible routes to achievement. Intensive psychotherapy is going to have little impact on the neurotic mother whose energies are consumed by the struggle to meet the minimum physical needs of herself and her children. Similarly, preschool programs which capture the form but not the content of some of the more advanced models are doomed to failure. Nor will good programs which are not followed by greatly strengthened primary, elementary and secondary school programs make a major difference in the lives of these children. Improved and expanded mental health services will mean little unless our nation comes to grips with the problems of economic, political and social opportunities for masses of disfranchised and alienated persons.

To honor our traditional concern and for the sake of the disadvantaged, it is essential to recognize the limitations of the current effort. If the products of serious research were as well represented in this effort as the good intentions, the enthusiasm, the "band-wagon hopping" and the grant hunting, we could be more hopeful that meaningful solutions would be found to the problems of the disadvantaged. Unfortunately, some of us viewing the current efforts are left with a nagging suspicion that the net result of many of these programs will be to provide (for those who choose to interpret it so) empirical evidence of fundamental inferiority in these populations we are trying so hard to help. When five or ten years from now the populations we now call disadvantaged are still at the bottom of the heap, those who only reluctantly acceded to the current attempts to help may revive their now dormant notions of inherent inferiority to explain why all the money and all the effort have failed to produce results. The more likely fact will be that we shall have failed to produce the desired results simply because we shall have failed to develop and apply the knowledge and the skill necessary to the task. Unless the issues are more sharply drawn we may not even then recognize the nature of our incompetencies. We see in retrospect that bleeding was an ineffective cure for the plague, not because the barber-surgeons did not know how to draw blood, but because they did not sufficiently understand the nature of the disease with which they were dealing.

To honor our commitments to science and professional service, we must understand the limitations of our knowledge and our practice. Much of what we do is based on the hopeful assumption that all human beings with normal neurological endowment can be developed for participation in the mainstream of our society. We believe this because we have seen many people from a great variety of backgrounds participate and because we want to believe it. But we do not yet have definite evidence to support our belief. We operate out of an egalitarian faith without knowing whether our goals are really achievable. Yet it must be our aim, not only as scientists and professional workers, but as humanitarians as well, to determine the potential of human beings for equality of achievement. If in the light of our most sophisticated and subtle evaluations, we conclude that such equality is not generally achievable, if in spite of the best we can do it seems likely that some of our citizens will remain differentiated by their own biology, then we shall merely have answered a persistent question. We will still have no evidence that group differences per se imply any inability on the part of particular individuals to meet the demands of society. We will then be able to turn our energies to helping individuals meet those demands. And if, on the other hand, as we believe, true equality of opportunity and appropriate learning experiences will result in equality of achievement, then we must so organize our professional services and our society that no person is kept from achieving that potential by our indifference to his condition, by the inadequacy or inappropriateness of our service, or by the impediments society deliberately or accidentally places in his path. It is not an unhopeful paradox that the only way we shall ever know whether equality of human achievement is possible is through providing for all our citizens, privileged and underprivileged, the kind of service and society that assumes it is possible and makes adequate provision for the same. As we pursue the "Great Society" let us not be misled by the plethora of activity or companions in the cause.

Five Years Later

At this time, I can only add, let us also not be lead to premature and erroneous conclusions with respect to teachability by prophets of intellectual doom, by supporters of racist concepts of intellectual potential, or by well-meaning scholars whose vision and perspective may be too narrowly focused. Teaching remains the primary responsibility of educators. Teachability cannot be determined by genetic analysis, it can only be determined by adequate teaching.

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Goldstein (Continued from page 7)

ments in which the typical variability of each of us is expressed without help or hindrance from the environment.

The discovery of insulin, the isolation of vitamin D, the production of tuberculostatic drugs, the uncovering and control of phenylketonuria are all those exceptional environmental changes which will make this interaction term significant. They indicate that environments everywhere are not merely supportive of hereditary potentialities, but can, at times, reverse deleterious effects. The great achievements of mankind lie in making that interaction term significant. Indeed, it could almost be a maxim for schools of education, psychology, public health, and medicine: "Make that interaction significant." It is in this sense that almost all geneticists, psychogeneticists and biochemical geneticists are interactionists. Jensen almost alone holds to a "threshold" theory of environmental action.

I need not go on further in this vein. I have not alluded to Jensen's substantive arguments. It is enough to say that his genetic arguments contain a misunderstanding of a difficult sort, scientifically, the sort of misunderstanding that says because everyone knows that **x** cannot happen, we should stop looking for it. The German philosopher Hegel said that the perfection of the heavens was contained in the existence of exactly seven planets, mathematically spaced, no more or less, and that further planets could not exist. Unfortunately, a new planet was shortly discovered. A similar fate has awaited every past claim that evidence of heritability meant that nothing more could be done about it, and it is late in the game, indeed, to be holding this position.

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